## Note on the Instrumental Errors affecting Observations of the Moon. By F. W. Dyson, M.A., F.R.S.

1. In connection with Mr. Cowell's comparison of the observed positions of the Moon with the tabular positions Professor Turner contributed a note to the *Monthly Notices* of 1904 March in which he urged the necessity for more elaborate discussion of the instrumental errors to which observations of the Moon are liable, and suggested the possible utility of the observations made with the old altazimuth at Greenwich for this purpose. My reason for offering any remarks on the subject is that I have had opportunities of discussing the matter with Mr. Cowell and may be able to remove some misconceptions.

The ordinary sources of systematic errors in star catalogues such as a period in the adopted clock-star list, the adopted value of the equinox correction, flexure, the adopted system of refractions, &c., are of secondary importance in the discussion of short-period terms in the Moon's tables.

(*a*) Because the Moon moves so rapidly in both right ascension and declination.

(*b*) Because they are largely overwhelmed ($\alpha$) by the personal errors in observations of limbs, ($\beta$) the errors arising from observations being sometimes made in a bright and sometimes in a dark sky, and ($\gamma$) the large accidental error arising from the limb being sometimes ill-defined and hazy.

2. The errors in transit-circle observations which are not peculiar to the Moon being capable of independent determination should be allowed for wherever their effect is likely to be appreciable. They will very rarely affect the terms of short period, but may be of importance in the terms of longer period.

3. Professor Turner's note as far as it criticised Mr. Cowell's procedure had reference to the errors peculiar to the Moon marked (*b*) in § 2. The errors of this class largely increase the accidental errors of an observation of the Moon over that of a star. Their effect will, however, be of an entirely accidental character on the coefficients of all short-period terms except those whose period is a lunation or an exact part of a lunation. Certainly in the long series of observations from 1750 to 1901 which Mr. Cowell is discussing we may expect them to be entirely eliminated.

4. Errors of this class have, as Mr. Cowell has been careful to note (*Monthly Notices*, lxiv. p. 94), a systematic effect on the parallactic inequality and on the variation. Mr. Cowell proposes to determine the coefficient of $\sin D$ and $\sin 2D$ separately for

each period of 400 days from 1750–1901, and to obtain from the accordance of the separate determinations a measure of their accuracy. In this long period there have been a good many observers and several instruments at Greenwich. Errors of a personal character which are systematic in a short series of observations may be fairly considered as accidental in a long one extending from 1750–1901. The detailed examination of these personal errors would probably increase the accordance of the results derived from the separate periods but not seriously affect the mean. It should be noted that such a discussion would not be possible or would, at least, be very rough till the tabular errors which Mr. Cowell is considering are allowed for. At the best personal errors are troublesome to investigate, and the results never free from the uncertainty that observers may have slowly changed their habits of observing.

5. Mr. Cowell's method of giving the coefficients as determined separately for each period of 400 days gives a ready method of comparing the results as given by the different instruments used since 1750. If discrepancies are shown larger than can be legitimately attributed to accidental error they will be put clearly in evidence and their amounts will be known. But there are, as Mr. Cowell remarks, no means available for determining the instrumental correction which may be required by any particular instrument to the Moon's diameter at different ages and different conditions. The agreement of the observed with the theoretical variation and of the solar parallax obtained from the observations 1847–1901 with values obtained in other ways shows that the errors of this class are not very large for the present transit circle, and affords ground for supposing that their effect on other terms, except the parallactic inequality, is extremely small.

6. With regard to the altazimuth observations it is no doubt true that the systematic errors to which they are liable are probably different from those inherent in meridian observations. Unfortunately they have a large accidental error, and quite probably large systematic errors, depending directly on the Moon's altitude, and thus indirectly on its age. The comparison of the observed with the tabular places might be analysed. This would have the advantage over Professor Turner's comparison with the transit circle that all the observations could be used. Whether the results would repay the labour is, I think, a very open question.

7. Professor Turner uses the altazimuth observations as a standard to compare the old transit circle in use from 1847–1851 with the present one, 1851–1861. This comparison is, it seems to me, made better *via* the more uniform tabular places, and can be taken from Mr. Cowell's paper (*Monthly Notices*, vol. lxiv. p. 526, col. 6). The periods 86, 87, 88 in Mr. Cowell's paper correspond to the years 1847–1851, and the periods 90–99 to the years 1851–1861. I extract the figures for these periods.

S S 2

| 1847–1851. | | Period. | Coeff. of sin D. | 1851–1861. | Period. | Coeff. of sin D. |
|---|---|---|---|---|---|---|
| Period. | Coeff. of sin D. | | | Period. | | |
| 86 | $+0''.2$ | 90 | $-0''.5$ | 95 | $-0''.9$ | |
| 87 | $-1.1$ | 91 | $+0.5$ | 96 | $-1.2$ | |
| 88 | $-0.1$ | 92 | $-0.8$ | 97 | $-1.3$ | |
| | | 93 | $0.0$ | 98 | $-0.9$ | |
| | | 94 | $-0.5$ | 99 | $-0.1$ | |

The mean for the whole period 1847–1901 is $0''.0$. It does not seem to me that these figures give any evidence—though their weight is small—of a systematic difference between the observations with the old transit circle from 1847–1851 and those with the present instrument.

The negative values for periods 94–98 suggest some systematic error of the transit-circle observations during this period, but viewed with the whole series of periods to 1901 it is quite possible that the agreement of sign for these years is accidental.

8. The figures which Professor Turner gives (Table V., p. 411) show that a very liberal allowance must be made for accidental error. The figures he gives are as follows :

| Day. | Old. | New. | Day. | Old. | New. |
|---|---|---|---|---|---|
| 16 | $0''.0$ | $+0''.1$ | 21 | $-0''.7$ | $+0''.6$ |
| 17 | $-1.1$ | $-0.1$ | 22 | $-0.2$ | $+1.0$ |
| 18 | $+1.1$ | $0.0$ | 23 | $+0.7$ | $+0.8$ |
| 19 | $-0.2$ | $+0.4$ | 24 | $+0.1$ | $+0.8$ |
| 20 | $-1.1$ | $+0.2$ | 25 | $-0.5$ | $+1.2$ |

The figures in the column "Old" seem wholly accidental, although except for day 25 each of these figures is derived from the differences of from 25 to 50 observations made with the two instruments. The systematic character of the figures in the column "New," as far as it is caused by the transit circle, seems to agree with the error noted in § 7 as existing in Mr. Cowell's periods 94–98, and in that case would not occur in a comparison extending over a longer period.

9. In conclusion, although the value Mr. Cowell gave for the solar parallax seems to me to be true within the limits he assigned, I do not think any discussion of the errors of the observations will materially narrow the limits. Observations of the Moon, whether meridian, altazimuth, or occultations, will always have the "limb" difficulty.

The agreement between the value he finds and the value obtained in other ways seems to me to show that in a long series of observations the errors largely balance one another, and that as the parallactic inequality and the variation are the terms which are affected systematically, the accuracy of the determination of short-period terms not so affected will be very great.

*Comments on Mr. Dyson's " Note on the Instrumental Errors affecting Observations of the Moon."* By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

(*a*) Mr. Dyson courteously submitted the preceding paper to me in MS., and I offer the following comments upon it which it may be convenient to have in the same number as the paper itself.

(*b*) Paragraphs 1, 2, 3 of his note call for no remark from me.

(*c*) In § 4 our views begin to diverge, but only in the phrase " to obtain from the accordance of the separate determinations a measure of their accuracy." The danger of this test is well known even when all conceivable systematic errors have been removed. When there is an obvious source of systematic error still affecting the observations, I doubt whether the test can be applied. But we shall come across this point more definitely later.

(*d*) On § 5 I would urge that if there really are " no means available for determining the instrumental correction, &c., &c." —that is, if the solar parallax as determined from observations of the Moon is affected with an entirely unknown systematic error, then it is a pity to publish the result. To do so is quite likely to make trouble, from the value being carelessly used irrespective of the limitation on some occasion or another ; and that it is so near the probable truth only increases this likelihood. And what good is it ? We know that the solar parallax is there or thereabouts ; what we want to know now is, if possible, where *exactly* it is.

(*e*) On § 6 I have no comment.

On § 7 I have not yet before me the paper of Mr. Cowell's quoted, but I gather that the column referred to only gives the values of sin D. This only gives information concerning a very small part of the instrumental error for two reasons.

Firstly, if the instrumental error be expressed as a series

$$a_1 \sin D + b_1 \cos D + a_2 \sin 2D + b_2 \cos 2D + \text{ &c.}$$

the coefficient $b_1$ at any rate is likely to be large. For the instrumental error is almost certain to be quite different at new moon and full moon, *i.e.* at $D = 0°$ and $180°$. Now *sin D* has the same value at these points, and thus terms like $a_1 \sin D$ alone cannot represent errors of this kind. To trace changes in instrumental errors we must have at any rate the coefficients of cos D as well as those of sin D, and probably the higher coefficients also.

Secondly, *for expressing instrumental error* I do not trust the coefficients found in this way at all, for the reason that we have so few observations near new moon. It must be remembered that the case is totally different from that of the determi-